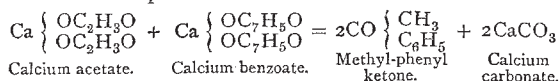
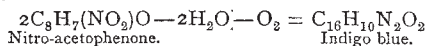


submitted to distillation in order to obtain the ketone known as acetophenone :—



Methyl-phenyl ketone when treated with fuming nitric acid yields two isomeric nitro-derivatives, $\text{C}_8\text{H}_7(\text{NO}_2)_2\text{O}$, one of which when heated with a reducing mixture composed of zinc dust and soda lime is converted into indigo blue :—



The process above given is at present only valuable from a scientific point of view, since the yield of indigotin is but small. It yet remains to convert this laboratory reaction into a practicable process, in order to do for indigotin what has already been accomplished for alizarin, and thus completely revolutionise another large branch of the colour-producing industry.

R. MELDOLA

OUR BOOK SHELF

Scientific Culture. By Josiah P. Cooke, Jun., Professor of Chemistry and Mineralogy in Harvard College (U.S.). (London: H. S. King and Co., 1876.)

THIS is altogether an admirable address, characterised by real eloquence and by clearness and decision of view as to the place which science ought to occupy in any system of education. Most of Prof. Cooke's audience were teachers by profession, attending Harvard University mainly to become acquainted with the experimental methods of teaching physical science. We commend the address not only to scientific students and teachers of science, but to all who take an interest in education, and to all students who desire a clear statement as to what, in the not distant future, will be regarded as the only liberal education, an education in which science will be allotted a place of at least equal importance with that of literature. What Mr. Cooke's views are on certain matters which have for long been discussed in this journal, may be learned from the following extracts. On the place which Science ought to occupy in education, he says :—

"I must declare my conviction that no educated man can expect to realise his best possibilities of usefulness without a practical knowledge of the methods of experimental science. If he is to be a physician, his whole success will depend on the skill with which he can use these great tools of modern civilisation. If he is to be a lawyer, his advancement will in no small measure be determined by the acuteness with which he can criticise the manner in which the same tools have been used by his own or his opponent's clients. If he is to be a clergyman, he must take sides in the great conflict between theology and science, which is now raging in the world, and unless he wishes to play the part of the doughty knight, Don Quixote, and think he is winning great victories by knocking down the imaginary adversaries which his ignorance has set up, he must try the steel of his adversary's blade. . . .

"I feel that any system of education is radically defective which does not comprise a sufficient training in the methods of experimental science to make the mass of our educated men familiar with this tool of modern civilisation; so that when, hereafter, new conquests over matter are announced, and great discoveries are proclaimed, they may be able not only to understand but also to criticise the methods by which the assumed results have been reached, and thus be in a position to distinguish between the true and the false. Whether we will

or not, we must live under the direction of this great power of modern society, and the only question is whether we will be its ignorant slave or its intelligent servant."

On the uses to which Universities should be put, Mr. Cooke's opinions are decided :—

"The time has passed when we can afford to limit the work of our higher institutions of learning to teaching knowledge already acquired. Henceforth the investigation of unsolved problems, and the discovery of new truth, should be one of the main objects at our universities, and no cost should be grudged which is required to maintain at them the most active minds in every branch of knowledge which the country can be stimulated to produce.

"I could urge this on the self-interest of the nation as an obvious dictate of political economy. I could say, and say truly, that the culture of science will help us to develop those latent resources of which we are so proud; will enable us to grow two blades of grass where one grew before; to extract a larger per cent. of metal from our ores; to economise our coal, and in general to direct our waiting energies so that they may produce a more abundant pecuniary reward. . . . This is all true, and may be urged properly if higher considerations will not prevail. It is an argument I have used in other places, but I will not use it here; although I gladly acknowledge the Providence which brings at last even material fruits to reward conscientious labour for the advancement of knowledge and the intellectual elevation of mankind. I would rather point to that far greater multitude who have worked in faith for the love of knowledge, and who have ennobled themselves and ennobled their nation, not because they have added to its material prosperity, but because they have made themselves and made their fellows more noble men."

These are but small samples of the many good things contained in Prof. Cooke's address, which we should like to see in the hands of all students. The latter portion of the address students of mineralogy will find of special value.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

On the most Northerly Latitude at which Land and Freshwater Molluscs have hitherto been found

I AM very sorry that I have involuntarily made a mistake in a letter to Mr. Oscar Dickson (*NATURE*, vol. xiii. p. 96), in which it is stated that Dr. Stuxberg had found a *Physa* on the most northerly locality from which land and freshwater molluscs have hitherto been obtained. When I made this statement I had not Middendorff's "Sibirische Reise" with me, and I did not then remember that this celebrated naturalist had found a species of the same genus on the Taimur peninsula north of the seventy-third degree N.L.

A. E. NORDENSKJÖLD

Stockholm, Jan. 29

Prof. Tyndall on Germs

HAVING commented elsewhere (*Lancet and Brit. Med. Journ.* Feb. 5) upon Prof. Tyndall's recent attempt to establish the truth of the Germ Theory of Disease, my remarks in your columns may be very brief.

Prof. Tyndall tells the public he has uniformly failed to obtain evidences of putrefaction in previously boiled organic infusions protected from contamination by atmospheric particles.

The following investigators have, however, with one or other fluid, been able to obtain such results :—

1. Schwann, *Isis*, 1837; Poggendorff's *Annalen*, 1837.
2. Mantegazza, *Giorn. dell. R. Ist. Lombard.*, t. iii., 1851.
3. Schroeder and Dusch, *Annal. de Chimie*, tome xli., 1854.
4. Schroeder, Liebzig's *Annalen*, cix., 1859, and *Chem. News*, vol. v., 1862.

5. Pouchet, *Hétérogénie*, 1859, and *Nouvelles Expériences*, &c., 1864.
6. Pasteur, *Ann. de Chimie*, 1862 (see pp. 60-62).
7. Joly and Musset, *Compt. Rend.*, 1861 and 1862.
8. Jeffries Wyman, *American Journal of Science*, vol. xxxiv., 1862, and vol. xlv., 1867.
9. Victor Meunier, *Compt. Rend.*, tome xli., 1865.
10. Child, *Proceed. of Roy. Soc.*, June 1864, and April 1865.
11. Hughes Bennett, *Ed. Med. Journ.*, 1868.
12. Cantoni, *Gaz. Med. Ital. Lombard.*, tome i., 1868.
13. Bastian, *NATURE*, 1870; *Modes of Origin*, &c., 1871; *The Beginnings of Life*, 1872.
14. Burdon Sanderson, *NATURE*, Jan. 8 and June 1873; *Med. Times and Gaz.*, Oct. 22, 1873.
15. Huizinga, *NATURE*, March 20, 1873, and Pflüger's *Archiv*, vols. vii. and viii.
16. Lankester and Pöde, *Proceed. of Roy. Soc.*, vol. xxi., 1873.
17. Roberts, *Phil. Trans.*, vol. clxiv., 1874.
18. Samuelson, Pflüger's *Archiv*, vol. vii., 1874.
19. Gscheidlen, quoted by Dr. Sanderson in *Academy*, July 10, 1875.

I have set down the names in order of time, and included my own amongst them because those mentioned after me have all confirmed my results with regard to the putrefaction of some fluids in hermetically sealed vessels, from which the air has been expelled by boiling; the very experiments, in fact, which Prof. Tyndall now endeavours to impeach by his own one hundred and thirty-nine failures.

Dr. Burdon Sanderson's well-known corroboration of the accuracy of my results may be here reproduced. He says (*NATURE*, January 8, 1873):—"The accuracy of Dr. Bastian's statement of fact with reference to the particular experiments now under consideration has been publicly questioned. I myself doubted it, and expressed my doubts, if not publicly, at least in conversation. I am content to have established—at all events, to my own satisfaction—that, by following Dr. Bastian's directions, infusions can be prepared which are not deprived of an ebullition of from five to ten minutes of the faculty of undergoing those chemical changes which are characterised by the presence of swarms of Bacteria, and that the development of these organisms can proceed with the greatest activity in hermetically sealed glass vessels, from which almost the whole of the air had been expelled by boiling."

And, if Prof. Tyndall and others wish to know how far these results have since been generally recognised as correct, reference may be made to a review of my work, "Evolution and the Origin of Life," by Dr. Burdon Sanderson, in the *Academy* of July 10, 1875. There, in reference to the confirmation which these experiments had received, and in relation to other work in connection with the question generally by Samuelson and Gscheidlen, Dr. Sanderson writes:—"As regards the trustworthy character of the experiments themselves, it will probably be a sufficient guarantee to most readers that they have been conducted under the immediate supervision of men like Pflüger and Hoppe-Seyler, who occupy the foremost rank as vital physicists. Those who are more especially interested in the subject will best satisfy themselves of the exactitude and completeness with which all the investigations have been carried out by reading for themselves the original papers."

Although Dr. Sanderson thus thoroughly recognises the fact (and knows that others do the same) that many boiled fluids will putrefy in closed vessels from which air has been expelled by boiling, it is well known that he is not willing to regard such facts as the proof of the occurrence of "spontaneous generation." He admits, indeed (*British Medical Journal*, February 13, 1875, p. 201), that I and others have shown that Bacteria in their "ordinary state" are killed by a temperature of about 140° F.; but, instead of accepting "spontaneous generation" as an explanation of the occurrence of living organisms in the vessels above referred to, he pleads in favour of the only other possible explanation, viz., a "latent vitality" in some Bacteria-germs not extinguishable by exposure for ten minutes or so to the influence of boiling water. I felt it my duty to refer to this hypothesis in my address at the Pathological Society last year (*British Medical Journal*, April 10, 1875, p. 473); but, whatever its worth may be, Dr. Sanderson, whose learning and knowledge of the whole question few will dispute, knows that this, or some such hypothesis, alone stands in the way of the acceptance of "spontaneous generation" as a proved reality.

Prof. Tyndall, however, seems to regard this hypothesis as undeserving of notice. He makes no sort of reference to it, and

agrees with me in thinking that Bacteria and their germs are decidedly killed by five minutes' boiling in organic infusions. He still further supports the view held by me, in opposition to that of M. Pasteur, that such a result follows both with alkaline and with acid infusions.

This may seem to many of my readers a rather remarkable finale when compared with Prof. Tyndall's own anticipations; but I feel thoroughly assured that those who understand the subject will see that, in the present stage of the controversy, no other conclusions of value are deducible from his recent experiments. He appears to have completely misapprehended the present state of knowledge on the question; he has uniformly failed to obtain results which are now firmly established; and, as regards the only question which is at present in dispute, he unhesitatingly coincides with me.

Feb. 7

H. CHARLTON BASTIAN

I HAVE read with great interest and pleasure Prof. Tyndall's paper on Germs. But I am troubled on one point. I am sure Prof. Tyndall will not think my difficulties unworthy of attention and removal, though I confess that I am only one of that unpretending class to whose enjoyment and instruction he has devoted so large a share of his valuable time. I am an outsider in scientific research; I delight to follow every investigation which tends to the development of science; but I have not the time, and, if time were available, perhaps I should find that I had not the skill to conduct experiments for myself. I have to trust—and I have seldom found myself misled by trusting—to the recorded experiments of men whose reputation has been established by prolonged and valuable work. I cannot willingly give up this trust, and yet there is one passage in Prof. Tyndall's paper which almost forces me to do so. He tells us that in 139 instances he boiled organic solutions in flasks which were then hermetically sealed, and that in no one case did putrefaction accompanied by Bacteria occur. The inference he draws from this "cloud of witnesses" is that Bacteria cannot be developed in flasks so treated.

Precisely the opposite conclusion appears to have been arrived at by Prof. Burdon Sanderson (*NATURE*, vol. vii. p. 180). He also tested organic fluids in flasks boiled and hermetically sealed, and he found that putrefaction, with swarms of Bacteria, frequently followed. He considered it established that the development of Bacteria could proceed with the greatest activity in hermetically sealed glass vessels previously subjected to boiling heat.

I observe that Prof. Tyndall suggests that such contradictory results may be explained by "errors of preparation or observation." No doubt they may, but it would be a great shock to my scientific faith to be driven to this theory to explain apparent discrepancies between such observers as Professors Tyndall and Sanderson. I cannot help, not only hoping, but believing that there must be some way of reconciling the experiments of two such eminent inquirers, and I should be much perplexed if I were compelled to form an opinion whether the supposed error, if it does exist, ought to be attributed to the one or the other.

Can Prof. Tyndall relieve me from the necessity of believing that either went astray in his work? May there not have been some variations in the conditions which would allow us to accept both sets of experiments as sound? On carefully comparing the two I find that Prof. Tyndall's experiments are far more numerous than those tried by Prof. Sanderson. On the other hand, I find that Prof. Sanderson describes with admirable precision all the details of his work. Perhaps it scarcely fell within the scope of Prof. Tyndall's discourse to descend to such minutiae, but it may very well be that a more particular description, such as that which Prof. Sanderson published, of his treatment of the hermetically-closed flasks—as to the preparation of the solutions, and their specific gravity, the mode and duration of the heating, the method and temperature of the developing treatment, and the like—would supply the means of reconciling his results with the apparently contradictory results arrived at by his brother professor.

When we are considering the conclusions of men of science of the highest calibre I do not think that over much weight should be attached to their preconceived expectations. Still, so far as they go, the avowed opinions of Prof. Sanderson before trying his experiments do tend to reinforce their value. He obtained results which he did not anticipate, and that after taking very careful precautions to exclude the possibility of errors of observation. The errors may have crept in notwithstanding, but it is especially uncomfortable to us outsiders to think